Logical and historical arguments are offered to prove that the following hypothesis is invalid: Given that in medieval China the study of magnetism had reached an advanced level, if it had had the sociohistorical condition to develop modern science, then China might have developed "field physics" (of electromagnetism) first and bypassed the Newtonian "billiard-ball physics." The fallacy of this hypothesis serves us as the starting point of developing some profound ideas—that is, those related to the evolutionary patterns of exact sciences.

Science Development

Sino-Western Comparative Insights

WEN-YUAN QIAN
Zhejiang University and
University of Michigan

> It *had* to go that way, if Herr Förster knew anything about composition.
> —Ludwig von Beethoven

No one denies that science has become one of the most precious common properties of all rational human beings. Even historically, the growth of science was not just a "European miracle": There were the Greek miracle, the Chinese miracles (an early one in the Warring States, the fourth and third centuries B.C., another in the Northern Song Dynasty, the tenth and eleventh centuries A.D.), the Arabic miracle, and the Western European miracle. However, owing to their great disparity in stature, some historians think, with cogent reasons,

---

Author's Note: Mr. W. Clement Stone's magnanimous support made possible my "leisurely" study. I am very grateful to him. As usual, I finished the writing of this article with interesting discussions with and cordial help (including criticism, of course) from my Northwestern colleagues: Professor William Monter, Professor James Sheridan, and Dr. Robert Michaelson. Professor David Joravsky is currently working in Europe, but his sincere and efficient guidance of my historical scholarship for more

Knowledge: Creation, Diffusion, Utilization, Vol. 6 No. 4, June 1985 377-405

that only the ancient Greek miracle and the post-Renaissance Western miracle, or only the last one, were true scientific "miracles." In this article I am talking about the growth of *modern* science—more specifically, modern physical, exact, and quantitative sciences (mathematics, physics, chemistry, biological taxonomy, anatomy, bacteriology, immunology, and so on).

Historical scholarship has recently discovered that a comparative study of historic phenomena that engendered or hindered the growth of modern science in China is a rich field of theoretical interest. Such study began with an inquiry into the causes that delayed the development of modern science in China. According to the Needham school, medieval China verged upon generating modern science autonomously. To many, especially Chinese scholars (Ren, 1915; Feng, 1922; He, 1983; Qian, 1983), an immediate question arises: Is this a justifiable statement? A careful study of this question naturally leads to a series of comparative pursuits, mainly Sino-Western, both in sociopolitical development and in the evolution (and revolutions) of physical sciences in particular.

The all-inclusive, almost banal, yet always unavoidable pair of questions are, What are the decisive factors that made the European "miracle" possible? and, What are the decisive factors that made a Chinese "miracle" impossible? In this article I will outline a theoretical answer to these two questions. But before that a number of preparatory concepts should be clarified and some questions answered. In this way I will summarize my theoretical conclusions reached as the result of a comparative study. Let me start with one important aspect of the history of science, an aspect which, I believe, has not yet received adequate attention. It can be called, for lack of a better phrase, the scientific character of science. For us, the significance of the scientific character of science is that it lends "uniqueness" or "determinism" to the historic development of science itself.

The words "uniqueness" and "determinism" are in quotation marks because they are not literally true. At any time there coexist dif-

---

than two years should also be acknowledged. Professor Burkart Holzner presided at the interesting session at which I presented my views and ideas to the 12th Annual Meeting of the ISCSC (International Society for the Comparative Study of Civilizations; Buffalo, May 1983). Thereafter he discussed with me in conversations and through long letters a number of comparative questions. I thank him for allowing me to benefit greatly from his sweeping discourse, and for encouraging me to write this summary. I am very grateful to Professor John King Fairbank for his warm and steady enouragement.

ferent sets of sciences; like everything, the "uniqueness" of science is relative. If we look at a wide spectrum of rational inquiries, institutions, and cultures, science is distinguished by its "immediate" demand of empirical verifiability and/or rigorous logic. (There is no question that the idea of science develops and crystallizes in the course of history, and so does the recognition of this immediate requirement.) Since empirical verification and/or rigorous logic eliminate *in principle* all but one possibility, one may call the scientific character of science "the criterion of scientific uniqueness."

In ordinary parlance we contrast "hard science" with social science. Economics is the "hardest" among the social sciences, yet it is still so "soft" that it allows different schools to debate at the level of daily talk. (By the way, I am confident that economics is being "hardened." Michaelson, my friend, remarked that what distinguishes hard sciences from social sciences is a shared expectation that in the former eventually a consensus will be reached. In the social sciences the same contending theories, perhaps somewhat modified, persist for centuries without either a synthesis or a triumph of one of them to produce consensus. What I want to add is that the shared expectation is based on historical experiences, in which the success of classical and modern physics played no small part.) According to the "uniqueness" criterion, different fields in exact or "hard" science can also be sorted by their degree of "uniqueness." There were only a few contending physical theories in each branch of physical phenomena before classical physics established a unifying theory in a classic manner; but in life and geological sciences, contending theories coexisted. (Indeed some are mandated on an equal-time basis by American law in a few states.) On "uniqueness" among exact sciences, Michaleson comments that the eliminative capability of empirical verification and/or rigorous logic should not be exaggerated even in the exact sciences. For instance, by this criterion for many decades theoretical astrophysics and theoretical particle physics were "soft" sciences. I agree with this point for developing sciences; on the other hand, I am confident about the applicability of the criterion in evaluating a longer period of the history of science.

In my judgment, the extent of success or failure (largely, failure) of the Marxian deterministic theory of social history is a result of two related macrohistorical facts: (1) Economic activities do not proceed in a unique manner—they are too much influenced by local natural conditions, cultural history, and current politics; and (2) politics depends too much on individual will and influence. (Consequently, economics lacks "uniqueness," or academic authoritativeness.)

I realized this criterion because in an authoritative book on the history of Chinese physics I was confronted with an explicit proposition that physics might have developed completely differently. Therefore, in a sense, I was forced to pick up the gauntlet—because I am a Chinese physicist. Nations may learn and follow science along sundry paths but positive scientific results acquired by scientific pathbreakers usually grow in a step-by-step manner. Fact-finding may be fortuitous and theoretical imagination idiosyncratic, but in building the edifice of science it ought to be the paramount concern of every scientific worker to try to fit every piece—an experimental effect or a logical link—into its proper place. There is no predetermined blueprint, but an unyielding rationalism guides the arduous solving of a gigantic jigsaw puzzle.

Therefore, myriad political and educational activities the world over do not invalidate the significance of the criterion, especially its historical significance. We are tackling the comparative history of science. On a phenomenological level we want to know: To what stage has science developed in a nation at a certain period? How much potential for developing modern science was there in a traditional society? For these and other related questions, this criterion of scientific "uniqueness" finds its meaningful applications. It was a passage on the first page of the volume that Needham devoted to Chinese physics (1962:1) that I found most challenging:

> Most important in this section will be, of course, the development of knowledge about magnetism, in particular the discovery and exploitation of the directive property of the lodestone. The Chinese were so much in advance of the Western world in this matter that we might almost venture the speculation that if the social conditions had been favourable for the development of modern science, the Chinese might have pushed ahead first in the study of magnetism and electricity, passing to field physics without going through the stage of "billiard-ball" physics. Had the Renaissance been Chinese and not European, the whole sequence of discoveries would probably have been entirely different.

Of course we noticed his apologetic tone in the wording of "might almost venture." But we should not dismiss this passage as insignificant. Because it is a stimulating hypothesis—why not examine it carefully? Moreover, in numerous passages in his books Needham (wrongly) ridicules "billiard-ball" physics, and he (wrongly) pits field physics against billiard-ball physics. For Needham, therefore, to bypass billiard-ball physics and to establish field physics directly is a rational reconstruction of historic development, not just a turn of whim. Besides, he thinks (wrongly) that the "replacement" of billiard-ball physics by field physics testifies to the victory of the organicist philosophy, which, according to him, is "rectifying" the mechanistic philosophy. He eulogizes the ancient Chinese proto-scientific thought, naming it "Chinese correlativism," and assumes that it will be "valuable for the future history of human thought," "essential for the construction of modern science in its present and coming form," because "natural science could not perfect itself without the characteristic philosophy of Chinese civilization" (Needham, 1956: 288, 339, 340). I will disclose Needham's misunderstanding of physics later. Indeed it was at once a shocking and revealing experience to find myself at odds with Needham on historical interpretation, physical understanding, and philosophical belief.

As I said before, different branches of science differ in their degree of uniqueness, and we should expect that physics, and mechanics in particular, exhibit a desirable degree of uniqueness. Historically mechanics was the first field of physics theorized. We acclaim Newton's achievements as the culmination of the Scientific Revolution because he explained a wide range of mechanical phenomena with a small bundle of axioms, or he carried out axiomatization. His methodological predecessors were Euclid and Descartes. His work itself set a model for all other branches of physics. Both pre- and post-Newton history of mechanics convince us that except in details mechanics could not have developed differently from its actual history. The post-Newton history illustrates the situation clearly. From the publication of the *Principia* in 1687 to 1905, the year that saw the publication of Einstein's special relativity, which changes the Newtonian concepts of space and time, all European mathematical geniuses could only reformulate Newton's system in different forms but not in essence. Owing to conceptual and mathematical difficulties, and the reliance upon a mature electrodynamics, it is simply inconceivable that special relativity—that is, another axiomatized system that deals successfully with mechanical locomotions—would have emerged before the Newtonian system. This historic inexorability is exactly what we mean by uniqueness of historic development.

In the early history of mechanics there was much less uniqueness in theoretical explanations. But the Newtonian unification rendered obsolete all contending theories. Similarly, in the development of electricity and magnetism, there appeared several different theories, one after another or contemporaneously. The situation was less messy in optical history, where there were basically two competing theories, corpuscular versus oscillatory. Finally by the end of the last century, classical physics succeeded in explaining the whole range of physical phenomena with a beautifully unified system.

Could electromagnetism have appeared ahead of mechanics as a mathematically axiomatized system? In actual history there was a two-hundred-year gap between the publication of the *Principia* and James Maxwell's *Electricity and Magnetism.* To every sober physicist the assumption of a reversed order of the establishment of these two theoretical systems of physical phenomena would be tantamount to admitting that it is a logical, or economical, or historical way of doing things by starting from a certain upper story to build a multistory building.

Apart from this logical mistake, the Needhamite hypothesis also blunders for obvious historical reasons. He speculates that with an advanced level of the science of magnetism, alongside social conditions favorable for the development of modern science, most probably (allowing Needham's apologetic tone) electromagnetism would have emerged without any aid from billiard-ball mechanics. To illustrate "favorable social conditions" he promptly uses the Renaissance as an example. But when Needham was saying all this he apparently forgot William Gilbert, his compatriot, who published a milestone book, *De magnete,* in 1600. There is no question that Gilbertian magnetism was much richer, more systematic, and intellectually more stimulating than the sum total of the contemporary Chinese knowledge on magnetism. Gilbert was a late Renaissance man—there is no question about this either. Since about 1600, in less than a century modern science grew rapidly in Western Europe and in England in particular. So, according to Needham's logic, the leading field of this modern science should be electromagnetism. But that is not the actual history. In bringing both mechanics and electromagnetism to fruition England played a vital role. It seems that Needham had also forgotten several other English scientists and their major contributions, which can be shown schematically in a double series:

W. Gilbert	I. Newton	H. Cavendish	M. Farraday	Lord Kelvin	J. Maxwell
magnetism	mathematical mechanics	electricity (no direct historic influence)	(mainly) experimental electromagnetism	(saliently) engineering electromagnetism	mathematical electromagnetism

The failure of Needham's hypothesis to conform to actual history accentuates the inexorability of the actual historic path of physics, which, according to our uniqueness idea, is deterministic in the basically ordered accumulation of scientific contents. With this criterion of the historic development of physics, and mechanics in particular, we can evaluate the corresponding situation in traditional China with great perspicacity.

Observations on mechanical phenomena began very early in China. Significant observations belong to two groups, one about buoyancy and the other about the lever. In ancient Greece, Archimedes scientifically summarized these two groups of observations into his two great principles. Therefore, we see a general Sino-Greek conformity with regard to basic mechanical phenomena that deservedly attracted scholarly attention, a conformity that supports our idea of scientific "uniqueness." But in China, as we will explain, these preliminary observations stayed in a scientifically rudimentary stage for more than 2000 years.

As early as in the fifth and fourth centuries B.C., Mo Ti, the author of the Mohist Canon, formulated his observation on the lever correctly, although his was still not a general and quantitative principle (Needham, 1962: 22):

> As for the steelyard, let a quantity of material and a weight be balanced, the distance between the fulcrum and the point where the material is suspended being shorter than the distance between the fulcrum and the point where the weight is suspended. This will then be the longer. If now to both sides the same weight is added, the weight must go down.

From time immemorial the Chinese steelyard had been made according to a general quantitative principle: When a lever is in balance, the product of a weight on the other side and its distance from the fulcrum equals the product of a weight on the other side and its distance from the fulcrum. But it seems that the best Chinese formulation that approached this Archimedes principle was that ancient Mohist one quoted above. One of China's greatest misfortunes was that historically the Mohist and other non-Confucian schools (the Legalists, the Logicians, etc.) were officially banned by two early campaigns, one around 220 B.C. (rather indiscriminatingly), and the other in the first century B.C. These institutional developments were, in my judgment, primarily responsible for the ensuing nondevelopment in scientific formulation in China. These were vitally important, because science means and deposits its power in axiomatic comprehension.

Three centuries after Mo Ti, a Han prince, Liu An, sponsored an encyclopedic work, *Huai Nan Zi,* which contains the following passage (Needham, 1962: 23):

> Therefore if one has the benefit of "position," a very small grasp can support a very large thing. That which is small but essential can control that which is wide and broad. So a beam only 10 *wei* long can support a house 1000 *jun* in weight; a hinge only 5 inches in length can control the opening and closing of a large gate. It does not matter whether the material is large or small. What matters is its exact position.

Needham thinks that this last passage indicates "a wide understanding" of "the whole theory of equilibria as stated by Archimedes." But this seems to be a doubtful contention. Just compare these two passages, one from Mo Ti and the other from Liu An: the latter is scientifically a retrogression, although rhetorically superior. The Mohist text strove to be general and quantitative, whereas by the wording of the *Huai Nan Zi* passage we do not even know the relative configuration of the beam and the house, or of the "5 inch hinge" and the "large gate."

Next the question of buoyancy. A recent Chinese paper (Dai, 1980) explained two very terse passages—one comprises ten Han characters, another seventeen—in the Mohist Canon and ascertained that they were about buoyancy and "conform to Archimedes' principle of floating bodies which was discovered later." One passage says, "An object of big body floats high on water. It is explained by equilibrium." Another says, "There is an equilibrium between the body and its submerged part. It is as if there is a five to one exchange." Later, in the Eastern Han Dynasty there was the famous episode of "Cao Chong weighing an elephant": The almighty prime minister Cao Cao was curious about the weight of an elephant. No one could tell. His son Cao Chong stepped forward and said, "That's not hard. Lead the elephant into a boat and mark how deep the boat sinks. Then instead of the elephant put in heavy rocks until the boat sinks to the same mark. The total weight of the rocks is what you want to know." We will analyze how near this formulation is to (or how far away from) Archimedes' principle of buoyancy.

Somewhat earlier than Cao Chong, also in the Han, occurs an interesting passage about the operation of a group of technicians who produced standard capacity, or volume (Needham, 1962: 39):

> The workers ... make measures of capacity. They purify by successive heating samples of metal (presumably copper) and tin, until there is no further loss of weight. Then they weigh them.

Biot translated what follows: "After they have weighed them, they proportionalize (or equalize) them, and after that they measure them" (Needham, 1962: 40).

Now Needham points out (1962: 40) that "Jiang Yong, however, had already suggested at the end of the eighteenth century" that the second action, the one translated by Biot as "proportionalizing," had meant in the Han era "weighing in water." By this explanation Needham states, "What the Han technicians were doing, therefore, was essentially what Archimedes did, namely to ascertain the proportions of an alloy by weighing in water as well as in air." Then he concludes: "Such an interpretation makes much more sense than Biot's." It is, however, easy to see that we can follow either Biot's translation or Jiang Yong's explanation, but neither will lead us to Needham's conclusion. Many present-day Chinese, including me, would have agreed with Biot before they learned about Jiang Yong. Since we are now informed about his interpretation of the character "zhun," let us follow him and examine Needham's logic.

First we notice that Archimedes could solve his question for two reasons. For one, he was a good mathematician. In order to fulfill the task assigned to him by King Hieron, Archimedes did not have to know "the Archimedes principle of buoyancy." What he needed to know was the volume of the crown, which could be measured by displaced water. (Therefore, taking a bath in a tub was indeed crucial to Archimedes' "Eureka.") On the other hand, with the aid of his principle, which asserts that the loss of weight of a body in a liquid equals the weight of the liquid which is displaced, the volume of the crown easily could be determined by weighing the crown in and out of water. Second, Archimedes knew that there were at most two components in the alloy. If there had been three or more components, he could have only affirmed whether there was adulteration or not, but he could not have ascertained the composition quantitatively. A set of two equations with more than two variables has an infinite set of solutions.

In the above quoted Chinese passage, obviously it was Needham who inserted "(presumably copper)" after the word "metal." If the Han technicians were interested in the compositional problem, they should be more specific about the "metal." And if there were more than two metals in an alloy, ascertaining its composition was simply impossible by weighing the resultant weights (in and out of water) or measuring the resultant volume.

The first part of the quotation tells us that those technicians were producing standard capacity (volume). For this purpose, what is the point of knowing the compostion of the alloy? From the second part of the quotation, according to Jiang Yong's explanation, what might be inferred is that the Han technicians mastered the Archimedes principle to the extent—still far from an exact and full formulation of the pinciple—that if the weights weighed the same in the air, and assuming they were of the same alloy compositions ("successive heating ... until there is no further loss of weight"), then they have the same capacity if they also weighed the same in water—consistent with their purpose of making standard capacity.

Apparently, I am not the only person to have noticed that Needham is sometimes inclined to overextend his extrapolations. Francis Hsu observed (1970: 371-372) that equal temperament "is commonly credited to the European Andreas Werkmesiter. But Needham after considering all sorts of evidence, infers that an independent invention of equal temperament in Europe is most unlikely." In Needham's own words:

> In any case it is fair to say that the European and modern music of the last three centuries may well have been powerfully influenced by a

> masterpiece of Chinese mathematics, though proof of transmission is not yet available.

In Needham's works one finds that he likes to repeat his unsubstantiated contentions again and again. In a recent book David Landes writes (1983: 20-23):

> Now this is optative history, the story of what should have been. There is, as Needham concedes, no evidence whatsoever linking the two horological tradtions. There is not even a bond of logic or method between them.
>
> In any event, neither the systemic differences nor the absence of corroborating evidence discouraged Needham. Using a *post hoc, ergo propter hoc* reasoning, he speculated that rumors of the Su Song clock somehow filtered westward over a period of a hundred or more years and struck a spark.

Landes helps us "to understand this figment, one has to place it in the larger context of Needham's philosophy of history ... what he calls ecumenical science." Finally let me quote Landes some more to conclude this insertional critique of Needham's manner of argument:

> This [the "ecumenical science"] is a noble credo, but it should not be allowed to get in the way of historical accuracy ... Joseph Needham is too great a scholar not to be taken seriously, but what does one do when his wishes become affirmation? Carlo Cipolla sees all this as fancy. I agree with him.

If the Han hydrostatics really attained the sophistication of using Archimedes' principle to ascertain the composition of a two-component alloy, then the concept of specific gravity should have been familiar to Han technicians and interested scholars. Yet it seems that written references to specific gravity or density in Chinese history were rare. Needham, however, believes that the "general idea of specific gravity must have existed from time immemorial." His reason (Needham, 1962: 39): "Mencius [fourth century] remarked that gold was heavier than feathers, otherwise how could it be said that a hook of gold was heavier than a cartload of feathers?" (Here we meet another example of Needham's method that justifies the critique shared by Landes, Hsu, and me.) After fifteen centuries, some written records that had something to do with the specific gravity or density of liquids emerged. The topic was the test of brine density, and the favorite testing probe was lotus seeds. Late in the eleventh century Yao Kuan wrote (Needham, 1962: 42):

> Everyday I tested the brine with lotus seeds. The heavier ones were selected for use. If brine can float 3 or 4 seeds (out of 10) it is considered strong brine. If it floats 5 (out of 10) it is the strongest. The seeds which float perpendicularly are preferred. If only 2 seeds float perpendicularly, or 1 perpendicularly and 1 horizontally, then the brine is considered thin and poor.

If all groups of 10 lotus seeds thus selected were identical statistical samplings (in terms of the specific gravity of seeds—a rough, rule of thumb hypothesis), the number of floats could show the specific gravity of the brine. But what had the specific gravity of brine to do with floating a lotus seed perpendicularly or horizonally? Indeed this whole device sounds more like a test of lotus seeds than of brine. There is definitely no science here, and if it is viewed as a technology, it is a false technology. But Chinese stagnation in mechanical science is featured more saliently by what Needham further tells us (1962: 42):

> Brine-testing methods of this kind are often afterwards referred to, with variations, as in the mid-12th-century *Neng Gai Zhai Man Lu* of Wu Cheng. And they have continued in use down to our own time.

In order to reach a general principle about weight reduction of an immersed body, Cao Chong's episode could serve as a propitious starting point. But several more steps—by no means easy ones—are needed. First, it should be decided that the weight of the water displaced by the elephant is equal (not just proportional) to the weight of the elephant. Then the procedure of weighing an elephant with a floating boat should be formulated in abstract terms: The total weight of a floating boat and the things in it is balanced by replacing an amount of water of equal weight. But a floating body is not an immersed body. Therefore, empirical knowledge on the latter was needed; according to Needham, the Chinese were familiar with it. But Needham's assertion is based on inconclusive and, indeed, very dubious explanations of what some Han technicians were doing. Next a general formulation of what is now accepted as the Archimedes' principle would be needed.

Finally, the principle should be explained theoretically—that is, in terms of hydrostatic pressure, a step that was accomplished before the middle of the seventeenth century in Western Europe. In other words, if Chinese hydrostatic development were to contribute to the "mechanical revolution,"—that is, to catch up with Western development up to the eve of the Newtonian culmination—it required (1) a general formulation about a floating body, (2) a general formulation about an immersed body, and (3) an explanation that unifies these and other hydrostatic facts. It is now clear that even with the most favorable interpretation of the practice of Han technicians, the actual Chinese situation in this respect can only be described as empiricist and underdeveloped, and it remained so for a long time.

While reviewing Needham's work, I found that my idea of Chinese inertia resonated unexpectedly with a notion that emerged in a different context. David Joravsky criticized Paul Feyerabend for praising the Maoist sponsorship of traditional Chinese medicine while keeping silent about its obvious follies. In order to illustrate some "critical questions" Joravsky (1979) used the case of "tadpole contraceptive":

> That rude defeat of belief by collision with physical reality raises critical questions. ... Was there some dreamy quality in the traditional Chinese mentality that permitted a persistent belief in a contraceptive method which could so easily be proved ineffective? And is there some harsher quality in the mentality of scientific physicians, some dream-destroying method, which persistently brings them closer than traditional rivals to objective tests of truth?

Have we not seen clearly this "dreamy quality in the traditional Chinese mentality" in the recognition of facts and principles about buoyancy? The situations are not exactly equivalent, because in the mechanical context some ideas were correct (such as Cao's weighing the elephant), some partly incorrect (such as testing the specific gravity of brine with lotus seeds). But, in the case of correct knowledge, no one bothered to generalize it. In the case of incorrect understanding, no one tested and criticized it. Anyone who has had direct contact with unlettered or little-educated Chinese peasantry knows that their major intellectual interest is to carry on with some old hearsays rather than to carry out experiments. This, I believe, is a universal mentality, which might be labeled "traditionalist." When Galileo carried two different weights to mount the Leaning Tower at Pisa—no matter whether the story was literally true or not—he behaved as a revolutionary. Somewhat later, in China, as Nathan Sivin (1982) tells us, when Wang Xi-chan, Mei Wen-ding, and Xue Feng-zo studied Jesuit planetary astronomy with a critical and creative attitude (Sivin, 1970b: 160), they also behaved as revolutionaries, although that does not mean that a "scientific revolution" occurred then in China.

If we take a bird's-eye view of the history of classical mechanics, we see clearly that its foundations were laid in three stages. The two Archimedes principles accomplished the first stage. Galileo's elucidation of a number of terrestrial gravity theorems and Kepler's three theorems of planetary movements fulfilled the second stage. Finally Newton's grasp of both terrestrial and celestial mechanical phenomena completed the historic mission that set a methodological paragon for other sciences. With this three-stage criterion we see that Chinese proto-mechanics reached a half-stage in approaching the phenomenological truth about buoyancy and the lever. Chinese scientists did almost nothing pertinent to the second stage of mechanical development, because Chinese astronomy was either too practical (calendrical) or overly disposed to note abnormalities (yi-xiang). A traditional astronomy that could not conceptually isolate our planetary solar system, can indeed boast very little. About Chinese planetary (or "explanatory") astronomy Needham writes (1962: 399):

> In spite of so much accurate observation, Chinese study of planetary motion remained purely non-representational in character. Unlike that of the Greeks, in which the geometry of circles and curves was so prominent, it perpetuated the algebraic treatment of the Babylonian astronomers such an Naburiannu and Kidinnu, and never sought a geometrical theory of planetary motions.

In his biography of Shen Kua, Nathan Sivin (1970a: 375) writes about the same problem:

> Prior to Shen's time little effort had gone into predicting the apparent motions of the planets, which lacked the immediacy of solar and lunar phenomena. This was, in fact, an omission that Shen seems to have been the first to confront.

Shen Kua was an eleventh-century polymath who once headed the Astronomical Bureau of the Northern Song Dynasty. The plan with which he and his collaborator Wei wanted to confront planetary astronomy was "exact coordinates read three times a night for five years." But owing to the "antagonism within the Bureau," such a plan could not be carried out. Consequently, "Shen and Wei had no recourse but to produce a conventional planetary theory based mainly on old observations" (Sivin, 1970a: 378).

For the major features of the subsequent development, let us quote Sivin again:

> Very gradually from the Yuan period (1279-1368), it came to be little practiced outside the Astronomical Bureau, which was dominated by foreign technicians. By 1600 no one was able fully to comprehend the old numerical equations of higher-order, prototrigonometric approximations, applications of the method of finite differences, and other sophisticated techniques.

Then came the age of Wang Xi-chan (Hsi-shan, 1628-1682), a hitherto comparatively obscure astronomer, but for recent sinophile revisionists one of the leading figures of the "Chinese scientific revolution" in the seventeenth century (Sivin, 1982: 62):

> Wang, Mei Wen-ding, and Xue Feng-zo were the first scholars in China to respond to the new exact science and to shape their influence on their successors. They were, in short, responsible for a scientific revolution. They radically reoriented the sense of how one goes about comprehending the celestial motions.

The general attainments of this scientific revolution are reflected on the one hand by a conjectural remark by Xi Ze-zong (Sivin, 1970b: 160):

> We can imagine, if Wang Xi-chan had only come upon Copernicus' *De revolutionibus,* Galileo's *Dialogue,* and Kepler's *Epitone astronomiae Copernicanae* (all of which the missionaries kept for their private use in Peking), how much greater his contribution to astronomy would have been

and on the other hand by the actual scientific performance (sorry, but "shabby" is the only word that occurs to me) in China during the past three centuries.

Since the 1960s "scientific revolutions" have proliferated in historiography. But I think the majority of these "revolutions" can better be called scientific crises that led to breakthroughs. Although Copernicus' great work is titled *De revolutionibus,* the seventeenth-century Scientific Revolution has been defined, with a different meaning for the word "revolution," as a sociohistorical movement comparable to or greater than the Renaissance and the Reformation that preceded it and the Enlightenment and the Industrial Revolution that followed it. So long as the alleged seventeenth-century Chinese "scientific revolution" was minute in both scope and depth, and in its sociohistoric impact, why call it so?

Nevertheless this episode may help us to illuminate some interesting points. Wang, Mei, and Xue's mastery of Western astronomy and calendrical knowhow, though brilliant, fell in a specialized realm. In a sense, their works embodied and anticipated the late nineteenth-century policy, or topic of debate: "Chinese learning is our substance; take Western learning as applications." Traditional China had always demanded a sophisticated calendrical science, and since the thirteenth century, employing foreign experts (Indians and Arabs) had probably been an uninterrupted tradition. Jesuit astronomers and their Chinese acolytes continued that tradition, which had never posed a threat to the "Chinese substance." A few generations before Wang, Mei, and Xue, Matteo Ricci's close collaborator, Xu Guang-qi, said: "Let us melt their (Western) material, and cast them into the mould of the traditional calendar" (Nakayama, 1973a: 132). Mei Wen-ding's false claim that the Westerners had learned Chinese mathematics sometime in high antiquity also reflects, in a way, the zeal to protect Chinese centralism, and ideologically, the "Chinese substance."

With regard to the macrohistoric pace of progress, one of my comparative insights is that "software decides." Three and a half centuries ago Francis Bacon eloquently stated that printing, gunpowder, and the magnetic compass had, more than anything else, given impetus to historic progress. Western historians after Bacon were surprised to realize that all three inventions might have originated in China. In this century another Englishman, Joseph Needham, declared (1969: 11) that for "not only the three which Lord Bacon listed ... but a hundred others—mechanical clockwork, the casting of iron, stirrups and efficient horse-harness" China enjoys the priority of invention. Equipped with these historiographical discoveries, Needham says (1969: 154), "the more you know about Chinese civilization, the more odd it seems that modern science and technology did not develop there." Needham identifies (1969: 154) this question as "one of the greatest problems in the history of culture and civilization—namely, the great problem of why modern science and technology developed in Europe and not in Asia." But as a Chinese reader I was very puzzled by this "Needham Puzzle." According to the general condition of China, past and present, had my ancestors come to the brink of generating modern science autonomously? My research into this question led to a flatly negative answer. Then I was confronted with another question: If traditional China enjoyed inventive priority for all those one hundred and more technologies, why did modern science not emerge in China? My conclusion was, and is, that those technologies are "hardware," whereas the decisive factors are "software"—dominant ideologies and politico-social institutions.

By logical inference, ironically the allegation of a "Chinese scientific revolution" supports our theorem that "software decides." The supposed Chinese seventeenth-century revolution made nothing truly different, except perhaps in a very small professional circle and for only a short period of time. (From the fact that since the 1920s leading astronomers at Chinese observatories have all been returned students from the West may I infer that there had not been a modernized *Chinese* astronomy in eighteenth and nineteenth centuries China?) In the wording of the nineteenth-century Chinese policy, the "substance" is our "software," and "applications" are our "hardware." If some items of "hardware" were modernized, or "revolutionalized," or more frankly Westernized, so long as the "substance"—that is, "software," remained unchanged the country was still its old self: Its general scientific condition could hardly be described as modernized. This was especially true in the past. Nowadays the basic domestic strategy of totalitarian nations is to keep their "substance"—their political institutions tightly combined with ideological controls—unchanged while trying hard to modernize their whole range of "hardware." Science does not grow wholesomely under totalitarian rule.

That "software decides" is one of my axioms. In explaining the history of mechanics, I stressed the three-stage "axiomatization" of of mechanical phenomena. By axiomatization I mean unified understanding through comprehensive principles. If we put ancient mechanical scientists in series such as Archimedes-Pascal-Newton, Aristotle-Stevin-Galileo-Newton, Ptolemy-Copernicus-Tycho, Brahe-

Kepler-Newton, we observe that one unified a wider range of mechanical phenomena than the one before him and the upshot was a small bundle of axioms—not necessarily self-evident, but very fertile to produce theoretical derivatives and very sturdy to withstand empirical and logical tests. In the realm of social studies, all post-Newton thinkers were influenced in varying degrees by Newton's (or Descartes', or Euclid's) axiomatic methodology. Jeremy Bentham proposed his Utilitarian Principle as *the* correct basis for understanding and guiding social phenomena; Adam Smith summarized the division of labor, laissez-faire, and so on. By citing these names I do not mean to propose exact axiomatization in sociohistorical study. The differences between physical and social phenomena are just too big; the former are characterized by mathematical determinism (here, philosophically, quantum-mechanical probabilistic laws included), and the latter are too much mixed with human free will, cultural history, and current conditions. On the other hand, as evidenced in history, the axiomatic line of thinking in social sciences may help us to identify some macrohistorical and hence significant causes and motifs. In the last part of the article, I will introduce another axiom, so as to honor my promise that I give and outline answers to our larger questions. We now discuss "axiomatization versus mathematization," which may also serve as an analogy to "software versus hardware."

Under the topic of ''Mathematics and Science in China and the West,'' Needham (1959: 154) stresses the emergence of modern science as a result of the mathematization of natural study in which Galileo occupied a central position. Galileo is contrasted with a long list of practitioners of the fifteenth and sixteenth centuries, among whom Leonardo da Vinci was the most outstanding. Needham thinks that Pledge hits the target when he says that in spite of Leonardo's deep insight into the nature and brilliance in experimentation, no significant development followed because of his lack of mathematics. Yet four pages later, Needham (1959: 158) also said that Strong

> had convincingly shown that before Galileo, and during his lifetime, mathematics was increasingly utilized by the practical technicians and artisans of whom we have already spoken. Some of these, such as Nicolo Tartaglia and Simon Stevin, were among the best mathematicians of their time.

Needham insists that a crucial step toward scientific progress was made by Galileo, and not by any of the previous practioners. Why Galileo? Because of his mathematics? (In fact, compared with Kepler, Descartes, and Newton, Galileo was obviously the least mathematical.) But his predecessors (after Leonardo, but before him) also had mathematics. As a result, we readers are left in a logical quandary. In order to resolve this quandary, I think we ought to be aware that "mathematization" was not the true "magic touch" (Needham, 1959: 154), but axiomatization was. Galileo and Tartaglia both did mathematization, but one did it in the realm of basic physical science, the other in the realm of technology. The operations of looking for measurables and hypothesizing their relations might be executed either on the level of basic phenomena, or on a derivative level. Science works on basic phenomena and arrives at conclusions which are universally valid but which may be remote from practical applications. That was the type of work Galileo and Kepler accomplished. They performed their respective axiomatizations, while Newton axiomatized both of theirs.

Since basic physical relations are quantitative, successful axiomatization necessarily implies mathematization. Galileo was very aware of this. He wrote that "philosophy" is written in mathematical terms and symbols. His younger contemporary Descartes was also very aware of this when he said he would take "god" and "the mathematical order of nature" as synonyms. Mathematized disciplines impress people with their accuracy and exactness. Yet an advanced axiomatization possesses even greater intellectual appeal. Newton's synthesis, of course, was well equipped with both exactness and comprehensiveness, which, in turn, impressed eighteenth-century philosophers and thus contributed conspicuously to the general progress of history. Science did not directly contribute to engineering technologies until about a century and a half after the publication of the *Principia*. But indirectly, the contribution of the Scientific Revolution—owing to a positive attitude toward intellectualism and rationality—was continuous and considerable.

The growth of modern science as a whole does not depend exclusively upon mathematization. At present, science as a whole is far from being totally mathematized, and will perhaps never be so. William Harvey's discovery of the circulation of blood formed a part of the Scientific Revolution of Galileo's time, yet it was not "written in the language of mathematics.'' But human intellect always tacitly accepts one fundamental assumption: other than seeking causal interconnections, explanation means accounting for a wide range of phenomena with a limited number of elements. The Yin and Yang, or the Five Elements, tried to work this way. They were the earliest instances of a physical synthesis in China. Nevertheless, in the subsequent long centuries of Chinese history, we seldom find the dimmest consciousness of scientific axiomatization. The first important case that comes to mind is the nonacceptance of Euclidean geometry.

Needham (1959: 105-106) has a succinct history of importing Euclid's *Elements* into China. About the probable thirteenth-century introduction, Needham writes:

> The evidence does not absolutely indicate that the books in question were a translation of Euclid into Chinese, though it seems probable that they were. In any case they had no perceptible effect, and during the following centuries the survey geometers, such as Tang Shunzhi and Zhou Shu-xue in the +16th century, continued quite naturally to follow the ways of their predecessors in the Tang and before.

After that, this great model of mathematical axiomatization was introduced (probably for the first time) in 1607 (the first six books), then in 1857, and 1865. Interestingly enough the last introduction was sponsored by the most prominent Confucian official of the time, Zen Guo-fan. It is conceivable indeed that as late as the 1860s, Zen Guo-fan might recommend Euclid, as he had done for the establishment of shipyards and arsenals. But he would never suggest the abolition of the examination system, which occurred 40 years later in 1905, after the humiliating Sino-Japanese War in 1894, the tragic 1898 Reform, and the disastrous Boxers' Rebellion in 1900. Zen Guo-fan's politicoideological stand could never surpass ''Zhong-xue wei ti, Xi-xue wei yong—Chinese learning is our substance; take Western learning as applications.''

For more than twenty centuries, the leading Chinese proto-scientific methodology was the so-called correlativism, which lacks exact logic, causal analysis, and physical explanations; whereas it emphasizes analogies, of which many were so remote and irrelevant that superstition is the only fitting name for them.

With regard to ancient explanations of earthquakes, Needham's section on seismology (1959: 624-625) contains a revealing comparison between an ancient Chinese theory and ancient Greek theories. The Chinese theory, which is described in the *Shi Ji (Records of a Historian* ending in 99 B.C.), says

> The dynasty of the Zhou is going to perish ... When the Yang is hidden and cannot come forth, or when the Yin bars its way and it cannot rise up, then there is what we call an earthquake. Now we see that the three rivers have dried up by this shaking; it is because the Yang has lost its place and the Yin has overburdened it. When the Yang has lost its rank and finds itself (subordinate to) the Yin, the springs become closed, and when this has happened the kingdom must be lost.

After this quote from the *Shi Ji,* Needham describes the continuous development of the theory of "the imprisoned Yang" until the Song Dynasty, as late as the end of the thirteenth century—and it might have persisted even longer. About ancient Greek theories Needham (1959: 625) uses Lone's summary:

> Anaxagoras believed that earthquakes were caused by excess of water from the upper regions bursting into the under parts and hollows of the earth; Democritus thought that this happened when the earth was already saturated with water, and Anaximenes suggested that the shocks were caused by masses of earth falling in cavernous places during the processes of drying. Aristotle himself in the 4th century attributed the instability to the vapour generated by the drying action of the sun on the moist earth, and to difficulties met with by the vapour in escaping.

Although Needham reminds his readers that Chinese theories were no more primitive than those the ancient Mediterranean world entertained on the subject of earthquakes, we could hardly help noticing two striking differences between the two sides. One is the strong political association in the Chinese natural theory. Consequently it acquires an authoritativeness that intends to compel people to accept their political conclusions. (Is this one of the keys to understanding the characteristic non sequitur in Chinese reasoning?) Another problem about the Chinese theory is the undefined "Yin and Yang." Looking at the terse summary of Greek theories, one admires by contrast their unadulterated naturalism: "excess of water," "saturated with water," "masses of earth," or "the vapour generated by the drying action of the sun."

I am aware of the long survival of such erroneous concepts as "phlogiston" and "ether" in Western scientific development. But they differ saliently from the Yin and Yang, because their physical and chemical functions and characteristics were well hypothesized. In the eighteenth century the "phlogiston," and as late as the beginning of this century the "ether," both played their parts in hectic times of experimental and argumentative sciences. When science pushed them out of its realm, as if by Newtonian reaction, they pushed science ahead. In China, the Yin and Yang appeared early, and were backed by political as well as intellectual authority. As if mired in a vicious circle, Chinese science could not expel them, hence could not make the necessary breakthrough.

The endurance of the idea of Yin and Yang ushers us into a debate on the scientific basis of scientific philosophies. Needham was described by some historians as an "organic philosopher" (Nakayama, 1973b), who saw "in Whiteheadian organismic philosophy the synthesis of mechanism and vitalism." According to the same author, Needham has a strong antipathy to the mechanistic view that characterizes many physical scientists, and he is inclined to synthesis over analysis. This is a useful link for us to understand why Needham found so much congeniality and merit in Chinese proto-scientific thinking. He has written numerous passages that state that the bureaucratic society of ancient and medieval China produced the philosophy of organism, which is according to him (as we have quoted before), valuable for the future history of human thought, or (to add one more quotation), which "may turn out to have been as necessary an element in the formation of the perfected world view of natural science" (Needham, 1956: 339). Yet herein Needham himself perceived a big irony (1956: 340):

> The gigantic historical paradox remains that although Chinese civilization could not spontaneously produce "modern" natural science, natural science would not perfect itself without the characteristic philosophy of Chinese civilization.

I, however, am very skeptical about this vision, which presents itself as a complete irony to continuity in scientific and philosophical progress. And on this issue Nakayama (1973b: 25) was as doubtful as I am:

> However, it is very doubtful that "organism" in its Chinese version could ever take the role of a promoter of modern science. Some of the very characteristics of organism that Needham considered uniquely Chinese might be found in other premodern cultures too.

Moreover, the physical, mathematical, and biological basis of Needham's organicist philosophy is very shaky. I cannot with full confidence evaluate his "endocrine orchestra" or "reticulate continuum" biology, but physics helps me to look at the situation from a vantage point, because physics has so far not only circumscribed but also penetrated biology successfully. If we have confidence in a unified philosophy about a unified universe which contains an *extremely small and thin wafer* of living phenomena, then what is true physically should be true throughout. Needham's misunderstanding of physics can be summarized this way: (1) He believes that Newtonian billiard-ball physics is scientifically wrong and historically unnecessary—as we have mentioned before; (2) correspondingly he believes that field physics replaces billiard-ball physics, and they contradict each other; (3) he believes that the relativistic concept of time supports his "timeless pattern" (specifically, he misunderstands the absolute conclusion of cause-effect relationship of special relativity as relative); (4) he separates geometry from algebra as methodologically—that is, on a philosophical level—two distinctive fields.

As I have analyzed before, Needham's idea is historically wrong, because he believes that human knowledge could have avoided the comprehension of mechanical phenomena as billiard-ball physics. It is not hard either to point out that he is also scientifically wrong. Conceptually, not only does field physics not contradict billiard-ball physics, but it is also a natural extension and development of the latter; a further confirmation of—to borrow Erwin Schrodinger's phrasing—"a naive physicist's comparatively simple and clear humble" ideal of ponderable, tangible and causally conceivable physics. By the way, quantum field physics does this job even better. Physically, billiard-ball physics holds true not only in a cosmological universe, in a human-size (or billiard-ball) universe, but also holds true in the deepest attainable micro-universe (many orders deeper than all the intrinsic biochemical mechanisms of living phenomena)—at all levels attainable by modern physical experimentation. One needs only to point out this simple and beautiful fact: all the millions of photographic records of elementary particle interactions demonstrate without exception that elementary particles interact like billiard-balls—variable billiard-balls subject to a set of conservation and nonconservation laws.

As the last topic we return to the question of the basic factors that made the "European miracle" possible and the basic impediments that inhibited science in China. This article starts with a discussion of the academic (or internal) aspect of science, which we term the scientific character of science. We are going to end the article with a discussion of the institutional aspect of science, which we may term the social character of science. We have already seen that mechanical science did not develop in China because some brilliant early observations on buoyancy and the lever were not carried over, examined, improved, and generalized by followers.

As early as the Mohist Canon, China already possessed a series of theorems on geometrical optics. But for more than a millenium after that, there was a historiographical blackout on the subject. Shen Kua, whom we mentioned before in relation to planetary astronomy, also studied optical phenomena, ignorant of Mohist conclusions. In the middle of the fourteenth century, Zhao You-qing carried out some brilliant experiments to prove the rectilinear propagation of light and to explain the image formed through a small hole, again unaware of his predecessors, either the Mohists or Shen Kua. In the realm of physical inquiry both the lack of professionalism and the lack of professional inheritance are evident. But we may also raise a related question: Does the lack of professional inheritance imply a lack of motivation to make deeper inquiries? Because traditional China did not provide the necessary intellectual zeal to sustain enduring systematic, rational, and causal inquiries about nature, we encounter repeated instances where some topics were picked up by interested scholars but then were subsequently disregarded. That is why Chinese geometrical optics, and physics in general, could never proceed beyond several elementary and imprecise statements.

In order to stay alive and vital, not only should science be carried on by the best brains of a sizable community, generation after generation, but these brains should be sufficiently motivated to work hard; to struggle against institutional hindrances and ideological limitations; to make sacrifices; to devote their whole lives; and to overcome intellectual biases and public prejudices. To take into account these factors, I have proposed another "axiom," the principle of the degree of intellectual activation and creativity. By the degree of activation we mean that we are referring to social phenomena and trying to talk in terms of statistical averages. As a criterion the primary concern of this principle is to see to what extent had conducive historical factors activated people's creative and intellectual efforts. And to what extent had these factors overcome inimical factors that would dampen the emergence of modern science? This principle accentuates science as a societal and multigenerational enterprise, modern science as an organized component of general modernity, and an institutionalized activity based on deep social consciousness and commitment. In my formal formulation of the principle (Qian, 1983) I mentioned the extensive, intensive, pluralistic, and inheritable aspects of intellectual activation, and I stated that the aim of activation is to promote constructive, innovative, explantory, and exploratory purposes. The early history of science was overburdened with myths about scientific wizards; in that type of history great scientists' endeavors and achievements were divorced from their socio-politial environment. But the principle of social activation will certainly more than remedy that drawback.

Even a perfunctory study of Western history of science impresses one with remarkably high ("degree" of) interest in the sixteenth and seventeenth centuries in natural, mathematical, and technological subjects. Pending a systematic study, let me just use one brief, "old," and excellent work, *Science and Social Welfare in the Age of Newton* (Clark, 1949). In the first dozen pages, Clark mentions the following scientific and technological publications: P. Virgil, *De inventoribus rerum* (1505, Paris; an abridgement in English was published in 1546); Ramus, *Scholae Mathematicae* (1569, Basel); F. Bacon's great works in the early seventeenth century; John Wilkins, *Mathematical Magic* (1648); Edward Somerset, *Century of Invention* (1663); and Sprate, *History of the Royal Society* (1667). Clark tells us that Somerset was not the only English nobleman who hired scientists and had his laboratory; the second Duke of Buckingham, King Charles himself, and Prince Rupert did the same.

With this backdrop, which may well be many times expanded, the contemporaneous famous accomplishment of Copernicus, Tycho Brahe, Gilbert, Kepler, Galileo, Harvey, and Newton, the formation of the first scientific societies, and the appearance of the first scientific journals—in short, the phenomenon of the Scientific Revolution—become more sensible.

As a sharp contrast, when in Ming China Song Ying-xing finished writing in 1636 a homely and pictorial book on ordinary productive technologies, he was painfully aware of the unmarketability of his brainchild. On this score, of course, we can also recall the abortive introduction of Euclidean geometry into China.

With this contrast we can understand how surprising it was when Chinese readers were recently taught by an American professor that there occurred a ''scientific revolution'' in seventeenth-century China. Indeed I think we really need to emphasize a ''Tolstoyan principle of historical commensurateness.'' I term it this way, because according to what I know this principle is Leo Tolstoy's invention; and as a guide to historical scholarship, again according to what I know, it is Geoffrey Parker's discovery (1972: ix). In Tolstoy's *War and Peace,* three peasants argued about what makes a locomotive move. Metaphorically the silly argument illustrates an important principle—to use Tolstoy's own formulation:

> The only conception capable of explaining the movement of the locomotive is that of a force commensurate with the movement observed. The only conception capable of explaining the movement of peoples is that of some force commensurate with the whole movement of the peoples.
>
> Yet to supply this conception various historians assume forces of entirely different kinds, all of which are incommensurate with the movement observed.

A historian who neglects this Tolstoyan principle is analogous to a physicist who forgets the law of energy conservation.

According to my knowledge the word ''revolution'' is generally reserved for historic-social changes of gigantic scale and momentous consequences—for example, the veritable ''price revolution'' in sixteenth-century Europe. Therefore, I would apply ''revolution'' to corresponding events and try to correlate them to commensurate causes and effects.

What can the combination of the two proposed ''axioms''—''software decides'' and ''degree of activation'' counts—tell us? In China, the decisive ''software'' that reduced the ''degree of modernity activation'' was traditional China's politico-ideological institution—a unified institution that dominated a densely populated and basically rural and vast territory. About the institution itself let me quote John K. Fairbank (1979: 392):

> This unitary system [the fusion of morality and politics] has worked in the People's Republic as under imperial Confucianism because moral-ideological authority and political power have been combined at the top.

My educated guess is that each of the few scholars who have recognized the significance of this decisive "software" has a Sino-Western comparative perspective at the back of his or her mind. I will borrow a passage from William Monter, my advisor, to explain this comparative perspective. Writing about my work, he expresses my idea with such superb elegance:

> If Qian is correct in his assumption that "software decides," then exactly what types of non-scientific "software" accompanied, facilitated, or at any rate failed to prevent the emergence of a recognizably "modern" science by the time of Newton? Cultural pluralism, says Qian. Considering that the human mass of China and Europe (including Western Russia) approximately balanced each other, the most important contrast he sees is between the politically and culturally unified Chinese empire, held together by a Confucian ideology administered by a bureaucracy equipped with a superb rote memory of classical texts—and the multiple divisions of Europe, where the loyalties of intellectuals were split between church and state, between state and state, and (after Luther) between church and church. In China a capricious, free-will "Son of Heaven" presided over an irregular natural world, but human affairs were usually well-ordered; in Christendom, a watchmaker—God—regulated Nature while human affairs swirled in a disorderly cauldron.

It was the Confucian ideal that human affairs be well-ordered under a "Son of Heaven," but the long history of traditional China has been characterized by a basic Chinese dichotomy: sociopolitical instability within ideologic-institutional continuity. The uniquely Chinese "dynastic cycles" with a relatively short average period witness the dichotomy.

For about a century and some more, China has been in a " translation movement." This is literally true in scientific and cultural matters, even to a considerable extent in various forms of official ideology, whose major representatives are Hong Xiu-quan, Sun Yat-sen, Mao Ze-dong, and Deng Xiao-ping. (As it can be seen, my macrohistorical evaluation is at the antipodes from either the stand that backs the concept of the "ecumenical science" or the one that proclaims that a "scientific revolution" occurred in seventeenth-century China.) But this Chinese translation movement differs remarkably from the one in medieval Europe, when European schoolmen translated proto-sciences and other classics. In a few centuries Western creativity surpassed the ancient originals and forged ahead (the fourteenth century was a scientific height). If China—with about one-fourth of the world population—wants to catch up with the West and forge ahead, it is vital to pay foremost (indeed, for many Chinese, *painful*) attention to "software" reformation, thus raising the degree of intellectual activation in a genuine, all-around, and enduring manner.

References

CLARK, G. N. (1949) Science and Social Welfare in the Age of Newton. Oxford: Oxford Univ. Press.

DAI, N. Z. (1978, 1980 reprint) in The Institute of the History of Natural Sciences, Academia Sinica (eds.) Zhang-quo Gu-dai Ke-xue Ji-shu Cheng-jiu (Scientific and Technological Achievements in China's Past). Beijing.

FAIRBANKS, J. K. (1979) The United States and China. Cambridge: Harvard Univ. Press.

FENG, Y. L. (1922) "Why China has no science?" Int. J. of Ethics, 32, 3.

HE, X. (1983) "Zhong Xi Xue-shu Cha-yi (Academic Disparity between China and the West)." J. of Dialectics of Nature, 5, 2. Beijing.

HSU, F.L.K. (1970) Americans and Chinese. New York: Natural History Press.

JORAVSKY, D. (1979) "Scientists as servants." New York Times Rev. of Books (June 28).

LANDES, D. (1983) Revolution in Time. Cambridge: Harvard Univ. Press.

NAKAYAMA, S. (1973a) "The empirical tradition," in A. Toynbee, (ed.) Half the World. London: Thames & Hudson.

———(1973b) "Joseph Needham: organic philosopher." in S. Nakayama and N. Sivin (eds.) Chinese Science: Exploration of an Ancient Tradition. Cambridge: MIT Press.

NEEDHAM, J. (1969) The Grand Titration. Toronto: Univ. of Toronto Press.

———(1956-1962) Science and Civilization in China, Vols. 2-4. Cambridge, England: Univ. Press.

PARKER, G. (1972) The Army of Flanders and the Spanish Road, 1567-1659. Cambridge: Cambridge Univ. Press.

QIAN, WEN-YUAN (1983) "The great inertia." Comparative Civilization Rev. 9 (Fall).
REN, H. J. (1915) "Shuo Zhong-guo we Ke-xue zhi Yuan-yin (Remarks on the Causes of China's Lack of Science)." Ke-xue (Science), Vol. 1.
SIVIN, N. (1982) "Why the Scientific Revolution did not take place in China—or didn't it?" Chinese Science 5 (June).
———(1970a) "Shan Kua," in C. C. Gillispie (ed.), Dictionary of Scientific Biography, Vol. XII. New York: Scribner's.
———(1970b) "Wang Hsi-shan." Ke-xue (Science), Vol. 14.

WEN-YUAN QIAN is a physicist from the Peoples Republic of China. He was a visiting scholar in the Department of History, Northwestern University. He is currently in the Department of History, University of Michigan, Ann Arbor.